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COSMICAL PHYSICS[1]

To one who has spent many years over the solution of a problem which is somewhat isolated from the more general questions of his subject, it is a satisfaction to have this opportunity for presenting the problem as a whole instead of in the piecemeal fashion which is necessary when there are many separate features to be worked out. In doing so, I shall try to avoid the more technical details of my subject as well as the temptation to enter into closely reasoned arguments, confining myself mainly to the results which have been obtained and to the conclusions which may be drawn from them.

In setting forth the present status of the problem, another side of it gives one a sense of pleasure. When a comparison between the work of the lunar theorist and that of the observer has to be made, it is necessary to take into consideration the facts and results obtained by astronomers for purposes not directly connected with the moon: the motions of the earth and planets, the position of the observer, the accuracy of star catalogues, the errors of the instruments used for the measurement of the places of celestial objects, the personality of the observers—all these have to be considered; in fact, almost every one of the departments of the astronomy of position must be drawn upon to furnish necessary data. The time has now arrived when it may perhaps be possible to repay in some measure the debt thus contracted by furnishing to the astronomer, and perhaps

[1] Address of the Vice-president of Section A, British Association for the Advancement of Science, Australasian meeting, 1914.

also to the student of geodesy and, if I may coin a word, of selenodesy, some results which can be deduced more accurately from a study of the moon's motion than in any other way. A long-continued exploration with few companions which ultimately leads to territories where other workers have already blazed paths gives the impression of having emerged from the thick jungle into open country. The explorer can once more join forces with his brother astronomers. He can judge his own results more justly and have them judged by others. If, then, an excuse be needed for overstepping the limits which seem, by silent consent, to have been imposed on those who devote themselves to lunar problems, it consists in a desire to show that these limits are not necessary and that a study of the motion of the moon can be of value and can contribute its share to the common funds of astronomy.

The history of the motion of the moon has been for more than two centuries a struggle between the theorists and the observers. Ever since the publication of the "Principia" and the enunciation of the law of gravitation by Isaac Newton, a constant effort has been maintained to prove that the moon, like the other bodies of the solar system, obeyed this law to its farthest consequences. While the theory was being advanced, the observers were continually improving their instruments and their methods of observing, with the additional advantage that their efforts had a cumulative effect: the longer the time covered by their observations, the more exact was the knowledge obtained. The theorist lacked the latter advantage: if he started anew he could only use the better instruments for analysis provided by the mathematician. He was always trying to forge a plate of armor which the observer with a gun whose power was increasing with the time could not penetrate. In the struggle the victory rarely failed to rest with the observer. Within the last decade we theorists have made another attempt to forge a new plate out of the old materials; whether we have substantially gained the victory must rest partly on the evidence I have to place before you to-day and partly on what the observer can produce in the near future.

There are three well-defined periods in the history of the subject as far as a complete development of the moon's motion is concerned. From the publication of the "Principia" in 1687, when Newton laid down the broad outlines, until the middle of the eighteenth century, but little progress was made. It seems to have required over half a century for analysis by symbols to advance sufficiently far for extensive applications to the problems of celestial mechanics. Clairaut and d'Alembert both succeeded in rescuing the problem from the geometrical form into which Newton had cast it and in reducing it to analysis by the methods of the calculus. They were followed by Leonard Euler, who in my opinion is the greatest of all the successors of Isaac Newton as a lunar theorist. He initiated practically every method which has been used since his time, and his criticisms show that he had a good insight into their relative advantages. A long roll of names follows in this period. It was closed by the publication of the theories of Delaunay and Hansen and the tables of the latter, shortly after the middle of the nineteenth century. From then to the end of the century the published memoirs deal with special parts of the theory or with its more general aspects, but no complete development appeared which could supersede the results of Hansen.

My own theory, which was completed a few years ago, is rather the fulfilment to the utmost of the ideas of others than a

new mode of finding the moon's motion. Its object was severely practical—to find in the most accurate way and by the shortest path the complete effect of the law of gravitation applied to the moon. It is a development of Hill's classic memoir of 1877. Hill in his turn was indebted to some extent to Euler. His indebtedness would have been greater had he been aware of a little-known paper of the latter, "Sur la Variation de la Lune," in which the orbit, now called the variation orbit, is obtained, and its advantages set forth in the words: "Quelque chimérique cette question j'ose assurer que, si l'on réussissoit à en trouver une solution parfaite on ne trouveroit presque plus de difficulté pour déterminer le vrai mouvement de la Lune réelle. Cette question est donc de la dernière importance et il sera toujours bon d'en approfondir toutes les difficultés, avant qu'on en puisse espérer une solution complète."

In the final results of my work the development aims to include the gravitational action of every particle of matter which can have a sensible effect on the moon's motion, so that any differences which appear between theory and observation may not be set down to want of accuracy in the completeness with which the theory is carried out. Every known force capable of calculation is included.

So much for the theory. Gravitation, however, is only a law of force: we need the initial position, speed and direction of motion. To get this with sufficient accuracy no single set of observations will serve; the new theory must be compared with as great a number of these as possible. To do this directly from the theory is far too long a task and, moreover, it is not necessary. In the past every observation has been compared with the place shown in the "Nautical Almanac" and the small differences between them have been recorded from day to day. By taking many of these differences and reducing them so as to correspond with differences at one date, the position of the moon at that date can be found with far greater accuracy than could be obtained through any one observation. At the Greenwich Observatory the moon has been observed and recorded regularly since 1750. With some 120 observations a year, there are about 20,000 available for comparison, quite apart from shorter series at other observatories. Unfortunately these observations are compared with incorrect theories, and, in the early days, the observers were not able to find out, with the accuracy required to-day, the errors of their instruments or the places of the stars with which the moon was compared. But we have means of correcting the observations, so that they can be freed from many of the errors present in the results which were published at the time the observations were made. We can also correct the older theories. They can be compared with the new theory and the differences calculated: these differences need not even be applied to the separate observations, but only to the observations combined into properly chosen groups. Thus the labor involved in making use of the earlier observations is much less than might appear at first sight.

For the past eighteen months I have been engaged in this work of finding the differences between the old theories and my own, as well as in correcting those observations which were made at times before the resources of the astronomer had reached their present stage of perfection. I have not dealt with the observations from the start: other workers, notably Airy in the last century and Cowell in this, have done the greater part of the labor. My share was mainly to carry theirs a stage further by adopting the latest theory and the best modern practise for the reduction of the

observations. In this way a much closer agreement between theory and observation has been obtained, and the initial position and velocity of the moon at a given date are now known with an accuracy comparable with that of the theory. I shall shortly return to this problem and exhibit this degree of accuracy by means of some diagrams which will be thrown on the screen.

I have spoken of the determination of these initial values as if it constituted a problem separate from the theory. Theoretically it is so, but practically the two must go together. The increase in accuracy of the theory has gone on successively with increase in accuracy of the determination of these constants. We do not find, with a new theory, the new constants from the start, but corrections to the previously adopted values of these constants. In fact, all the problems of which I am talking are so much interrelated that it is only justifiable to separate them for the purposes of exposition.

Let us suppose that the theory and these constants have been found in numerical form, so that the position of the moon is shown by means of expressions which contain nothing unknown but the time. To find the moon's place at any date we have then only to insert that date and to perform the necessary numerical calculations. This is not done directly, on account of the labor involved. What are known as "Tables of the Moon's Motion" are formed. These tables constitute an intermediate step between the theory and the positions of the moon which are printed in the "Nautical Almanac." Their sole use and necessity is the abbreviation of the work of calculation required to predict the moon's place from the theoretical values which have been found. For this reason, the problem of producing efficient tables is not properly scientific: it is mainly economic. Nevertheless, I have found it as interesting and absorbing as any problem which involves masses of calculation is to those who are naturally fond of dealing with arithmetical work. My chief assistant, Mr. H. B. Hedrick, has employed his valuable experience in helping me to devise new ways of arranging the tables and making them simple for use.

A table is mainly a device by which calculations which are continually recurring are performed once for all time, so that those who need to make such calculations can read off the results from the table. In the case of the moon, the tables go in pairs. Each term in the moon's motion depends on an angle, and this angle depends on the date. One table gives the value of the angle at any date (a very little calculation enables the computer to find this), and the second table gives the value of the term for that angle. As the same angles are continually recurring, the second table will serve for all time.

We can, however, do better than construct one table for each term. The same angle can be made to serve for several terms and consequently one table may be constructed so as to include all of them. In other words, instead of looking out five numbers for five separate terms, the computer looks out one number which gives him the sum of the five terms. The more terms we can put into a single table the less work for the astronomer who wants the place of the moon, and therefore the more efficient the tables. A still better device is a single table which depends on two angles, known as a double-entry table; many more terms can usually be included in this than in a single-entry table. The double interpolation on each such table is avoided by having one angle the same for many double-entry tables and interpolating

for that angle on the sum of the numbers extracted from the tables.

The problem of fitting the terms into the smallest number of tables is a problem in combinations—something like a mixture of a game at chess and a picture-puzzle, but unlike the latter in the fact that the intention is to produce ease and simplicity instead of difficulty. This work of arrangement is now completed and, in fact, about five sixths of the calculations necessary to form the tables are done; over one third of the copy is ready for the printer, but, owing to the large mass of the matter, it will take from two to three years to put it through the press. The cost of performing the calculations and printing the work has been met from a fund specially set aside for the purpose by Yale University.

A few statistics will perhaps give an idea of our work. Hansen has 300 terms in his three coordinates, and these are so grouped that about a hundred tables are used in finding a complete place of the moon. We have included over 1,000 terms in about 120 tables, so that there are on the average about eight terms per table. In one of our tables we have been able to include no less than forty terms. Each table is made as extensive as possible in order that the interpolations—the bane of all such calculations—shall be easy. The great majority of them involve multiplications by numbers less than 100. There are less than ten tables which will involve multiplications by numbers between 100 and 1,000 and none greater than the latter number. The computer who is set to work to find the longitude, latitude and parallax of the moon will not need a table of logarithms from the beginning to the end of his work. The reason for this is that all multiplications by three figures or less can be done by Crelle's well-known tables or by a computing machine. But Mr. Hedrick has devised a table for interpolation to three places which is more rapid and easy than either of these aids. It is, of course, of use generally for all such calculations, and arrangements are now being made for the preparation and publication of his tables. The actual work of finding the place of the moon from the new lunar tables will, I believe, not take more time—perhaps less—than from Hansen's tables, as soon as the computer has made himself familiar with them. Fortunately for him, it is not necessary to understand the details of their construction: he need only know the rules for using them.

I am now going to show by means of some diagrams the deviations of the moon from its theoretical orbit, in which, of course, errors of observation are included. The first two slides exhibit the average deviation of the moon from its computed place for the past century and a half in longitude.[2] The averages are taken over periods of 414 days and each point of the continuous line shows one such average. The dots are the results obtained by Newcomb from occultations; the averages for the first century are taken over periods of several years, and in the last sixty years over every year. In both cases the same theory and the same values of the constants have been used. Only one empirical term has been taken out—the long-period fluctuation found by Newcomb having a period of 270 years and a coefficient of 13″. I shall show the deviations with this term included, in a moment.

The first point to which attention should be drawn is the agreement of the results deduced from the Greenwich meridian observations and those deduced from occultations gathered from observatories all over the world. There can be no doubt that the fluctuations are real and not due to errors

[2] *Monthly Notices R.A.S.*, Vol. 73, plate 22.

of observation. A considerable difference appears about 1820, for which I have not been able to account, but I have reasons for thinking that the difference is mainly due to errors in the occultations rather than in the meridian values. In the last sixty years the differences become comparatively small, and the character of the deviation of the moon from its theoretical orbit is well marked. This deviation is obviously of a periodic character, but attempts to analyze it into one or two periodic terms have not met with success; the number of terms required for the purpose is too great to allow one to feel that they have a real existence, and that they would combine to represent the motion in the future. The straight line character of the deviations is a rather marked peculiarity of the curves.

The actual deviations on a smaller scale are shown in the next slide; the great empirical term has here been restored and is shown by a broken line. The continuous line represents the Greenwich meridian observations; the dots are Newcomb's results for the occultations before 1750, the date at which the meridian observations begin. With a very slight amount of smoothing, especially since 1850, this diagram may be considered to show the actual deviations of the moon from its theoretical orbit.

The next slide shows the average values of the eccentricity and of the position of the perigee.[3] The deviations are those from the values which I have obtained. It is obvious at once that there is little or nothing systematic about them; they may be put down almost entirely to errors of observation. The diminishing magnitude of the deviations as time goes on is good evidence for this; the accuracy of the observations has steadily increased. The coefficient of the term on which the eccentricity depends is found with a probable error of $0''.02$, and the portion from 1750 to 1850 gives a value for it which agrees with that deduced from the portion 1850 to 1901 within $0''.01$. The eccentricity is the constant which is now known with the highest degree of accuracy of any of those in the moon's motion. For the perigee there was a difference from the theoretical motion which would have caused the horizontal average in the curve to be tilted up one end over $2''$ above that at the other end. I have taken this out, ascribing it to a wrong value for the earth's ellipticity; the point will be again referred to later. The actual value obtained from the observations themselves has been used in the diagram, so that the deviations shown are deviations from the observed value.

The next slide shows the deviations of the mean inclination and the motion of the node, as well as of the mean latitude from the values deduced from the observations.[4] In these cases the observations only run from 1847 to 1901. It did not seem worth while to extend them back to 1750 for it is evident that the errors are mainly accidental, and the mean results agreed so closely with those obtained by Newcomb from occultations that little would have been gained by the use of the much less accurate observations made before 1847. The theoretical motion of the node differs from its observed value by a quantity which would have tilted up one end of the zero line about $0''.5$ above the other; the hypothesis adopted in the case of the perigee will account for the difference.

The mean latitude curve is interesting. It should represent the mean deviations of the moon's center from the ecliptic; but

[3] Tables II., III. of a paper on "The Perigee and Eccentricity of the Moon," *Monthly Notices R.A.S.*, March, 1914.

[4] "The Mean Latitudes of the Sun and Moon," *Monthly Notices R.A.S.*, January, 1914; "The Determination of the Constants of the Node, the Inclination, the Earth's Ellipticity, and the Obliquity of the Ecliptic," *ib.*, June, 1914.

it actually represents the deviations from a plane 0″.5 below the ecliptic. A similar deviation was found by Newcomb. Certain periodic terms have also been taken out. The explanation of these terms will be referred to directly.

The net result of this work is a determination of the constants of eccentricity, inclination, and of the positions of the perigee and node with practical certainty. The motions of the perigee and node here agree with their theoretical values when the new value of the earth's ellipticity is used. The only outstanding parts requiring explanation are the deviations in the mean longitude. If inquiry is made as to the degree of accuracy which the usual statement of the gravitation law involves, it may be said that the index which the inverse square law contains does not differ from 2 by a fraction greater than 1/400,000,000. This is deduced from the agreement between the observed and theoretical motions of the perigee when we attribute the mean of the differences found for this motion and for that of the node to a defective value of the ellipticity of the earth.

I have mentioned the mean deviation of the latitude of the moon from the ecliptic. There are also periodic terms with the mean longitude as argument occurring both in the latitude and the longitude. My explanation of these was anticipated by Professor Bakhuysen by a few weeks. The term in longitude had been found from two series of Greenwich observations, one of 28 and the other of 21 years, by van Steenwijk, and Professor Bakhuysen, putting this with the deviations of the mean latitude found by Hansen and himself, attributed them to systematic irregularities of the moon's limbs.

What I have done is to find (1) the deviation of the mean latitude for 64 years, (2) a periodic term in latitude from observations covering 55 years, and (3) a periodic term in longitude from observations covering 150 years, the period being that of the mean longitude. Further, if to these be added Newcomb's deviations of the mean latitude derived (*a*) from immersions and (*b*) from emersions, we have a series of five separate determinations—separate because the occultations are derived from parts of the limb not wholly the same as those used in meridian observations. Now all these give a consistent shape to the moon's limb referred to its center of mass. This shape agrees qualitatively with that which may be deduced from Franz's figure.

I throw on the screen two diagrammatic representations[5] of these irregularities obtained by Dr. F. Hayn from a long series of actual measures of the heights and depths of the lunar formations. The next slide shows the systematic character more clearly. It is from a paper by Franz.[6] It does not show the character of the heights and depths at the limb, but we may judge of these from the general character of the high and low areas of the portions which have been measured and which extend near to the limbs. I think there can be little doubt that this explanation of these small terms is correct, and if so it supplies a satisfactory cause for a number of puzzling inequalities.

The most interesting feature of this result is the general shape of the moon's limb relative to the center of mass and its relation to the principle of isostasy. Here we see with some definiteness that the edge of the southern limb in general is further from the moon's center of mass than the northern. Hence we must conclude that the density at least of the crust of the former is less than that of the latter, in accordance with

[5] *Abh. der Math.-Phys. Kl. der Kön. Sächs. Ges. der Wiss.*, Vols. XXIX., XXX.

[6] *Königsberger Astr. Beob.*, Abth. 38.

the principle mentioned. The analogy to the figure of the earth with its marked land and sea hemispheres is perhaps worth pointing out, but the higher ground in the moon is mainly on the south of its equator, while that on the earth is north. Unfortunately we know nothing about the other face of the moon. Nevertheless it seems worth while to direct the attention of geologists to facts which may ultimately have some cosmogonic applications. The astronomical difficulties are immediate: different corrections for meridian observations in latitude, in longitude, on Mösting A, for occultations and for the photographic method, will be required.

I next turn to a question, the chief interest of which is geodetic rather than astronomical. I have mentioned that a certain value of the earth's ellipticity will make the observed motions of the perigee and node agree with their theoretical values. This value is $1/293.7 \pm .3$. Now Helmert's value obtained from gravity determinations is 1/298.3. The conference of "Nautical Almanac" Directors in 1911 adopted 1/297. There is thus a considerable discrepancy. Other evidence, however, can be brought forward. Not long ago a series of simultaneous observations at the Cape and Greenwich Observatories was made in order to obtain a new value of the moon's parallax. After five years' work a hundred simultaneous pairs were obtained, the discussion of which give evidence of their excellence. Mr. Crommelin, of the Greenwich Observatory, who undertook this discussion, determined the ellipticity of the earth by a comparison between the theoretical and observed values of the parallax. He found an ellipticity $1/294.4 \pm 1.5$ closely agreeing with that which I have obtained. Finally, Col. Clarke's value obtained from geodetic measures was 1/293.5. We have thus three quite different determinations ranging round 1/294 to set against a fourth determination of 1/298. The term in the latitude of the moon which has often been used for this purpose is of little value on account of the coefficient being also dependent on the value of the obliquity of the ecliptic; such evidence as it presents is rather in favor of the larger value. I omit Hill's value, obtained from gravity determinations, because it is obviously too large.

Here, then, is a definite issue. To satisfy the observations of the moon in at least three different parts, a value near 1/294 must be used; while the value most carefully found from gravity determinations is 1/298. As far as astronomy is concerned, the moon is the only body for which a correct value of this constant is important, and it would seem inadvisable to use a value which will cause a disagreement between theory and observation in at least three different ways. It is a question whether the conference value should not be changed with the advent of the new lunar tables.

In looking forward to future determinations of this constant, it seems to be quite possible that direct observations of the moon's parallax are likely to furnish at least as accurate a value of the earth's shape as any other method. This can be done, I believe, much better by the Harvard photographic method than by meridian observations. Two identical instruments are advisable for the best results, one placed in the northern and the other in the southern hemisphere from 60° to 90° apart in latitude and as nearly as possible on the same meridian. On nights which are fine at both stations, from fifteen to twenty pairs of plates could be obtained. In a few months it is probable that some 400 pairs might be obtained. These should furnish a value for the parallax with a probable error of about $0''.02$ and a value for the ellipticity within half a unit of the denom-

inator 294. It would be still more interesting if the two instruments could be set up on meridians in different parts of the earth. The Cape and a northern observatory, Upsala for example, would furnish one arc; Harvard and Arequipa or Santiago another. If it were possible to connect by triangulation Australia with the Asiatic continent, a third could be obtained near the meridian of Brisbane. Or, accepting the observed parallax and the earth's ellipticity, we could find by observation the lengths of long arcs on the earth's surface with high accuracy.

In any case, I believe that the time must shortly come when the photographic method of finding the moon's place should be taken up more extensively, whether it be used for the determination of the moon's parallax and the earth's ellipticity or not. The Greenwich meridian observations have been and continue to be a wonderful storehouse for long series of observations of the positions of the sun, moon, planets and stars. In the United States, Harvard Observatory has adopted the plan of securing continuous photographic records of the sky with particular reference to photometric work. Under Professor Pickering it will also continue the photographic record of the moon's position as long as arrangements can be made to measure the plates and compute the moon's position from them.

In spite of the fact that Harvard Observatory has undertaken to continue for the present the work of photographing the moon's position, I believe that this method should find a permanent home in a national observatory. It has already shown itself capable of producing the accuracy which the best modern observations of Greenwich can furnish, and no higher praise need be given. If this home could be found in the southern hemisphere, and more particularly in Australia, other advantages would accrue.

But we should look for more than this. In an observatory whose first duty might be the securing of the best daily records of the sky, the positions of the sun, stars, planets, a couple of plates of the moon on every night when she is visible would be a small matter. What is needed is an organization so constructed as to be out of the reach of changing governmental policy with a permanent appropriation and a staff of the highest character removed from all political influences. It could render immense service to astronomers, not only in the Empire but all over the world. The pride which every Englishman feels who has to work with the records of the past furnished by Greenwich would in course of time arise from the work of a similar establishment elsewhere. Those of us who live in a community which, reckoning by the age of nations, is new, know that, in order to achieve objects which are not material, sacrifices must be made; but we also know that such sacrifices are beneficial, not only in themselves, but as exerting an indirect influence in promoting the cause of higher education and of scientific progress in every direction. In saying this I am not advocating the cause of the few, but of the majority; the least practical investigations of yesterday are continually becoming of the greatest practical value to-day.

No address before this section is complete without some speculation and a glance towards the future. I shall indulge in both to some small extent before closing. I have shown you what the outstanding residuals in the moon's motion are: they consist mainly of long-period fluctuations in the mean longitude. I have not mentioned the secular changes because the evidence for them does not rest on modern observations but on ancient eclipses, and these are matters too debatable to discuss in the limited time allotted to me for this address. It may be said, however, that the only

secular motion which is capable of being determined from the modern observations and is not affected by the discussion of ancient eclipses—namely, the secular motion of the perigee—agrees with its theoretical value well within the probable error. With this remark I pass to the empirical terms.

These unexplained differences between theory and observation may be separated into two parts. First, Newcomb's term of period between 250 and 300 years and coefficient 13″, and, second, the fluctuations which appear to have an approximate period of 60 to 70 years. The former appears to be more important than the latter, but from the investigator's point of view it is less so. The force depends on the degree of inclination of the curve to the zero line or on the curvature, according to the hypothesis made. In either case the shorter period term is much more striking, and, as I have pointed out on several occasions, it is much more likely to lead to the sources of these terms than the longer period. It is also, at least for the last sixty years, much better determined from observation, and is not likely to be confounded with unknown secular changes.

Various hypotheses have been advanced within the last few years to account for these terms. Some of them postulate matter not directly observed or matter with unknown constants; others, deviations of the Newtonian law from its exact expression; still others, non-gravitational forces. M. St. Blancat[7] examines a variety of cases of intramercurial planets and arrives at the conclusion that such matter, if it exists, must have a mass comparable with that of Mercury. Some time ago I examined the same hypothesis and arrived at similar results. The smallest planet with density four times that of water, which would produce the long inequality, must have a disc of nearly 2″ in its transit across the sun and a still larger planet would be necessary to produce the shorter period terms. But observational attempts, particularly those made by Perrine and Campbell, have always failed to detect any such planet, and Professor Campbell is of the opinion that a body with so large a disc could hardly have been overlooked. If we fall back on a swarm instead of a single body, we replace one difficulty by two. The light from such a swarm would be greater than that from a single body, and would therefore make detection more likely. If the swarm were more diffused we encounter the difficulty that it would not be held together by its own attraction, and would therefore soon scatter into a ring; such a ring can not give periodic changes of the kind required.

The shading of gravitation by interposing matter, *e. g.*, at the time of eclipses, has been examined by Bottlinger.[8] For one reason alone, I believe this is very doubtful. It is difficult to see how new periodicities can be produced; the periods should be combinations of those already present in the moon's motion. The sixty to seventy years' fluctuation stands out in this respect because its period is not anywhere near any period present in the moon's motion or any probable combination of the moon's periods. Indeed Dr. Bottlinger's curve shows this: there is no trace of the fluctuation.

Some four years ago I examined[9] a number of hypotheses. The motions of the magnetic field of the earth and of postulated fields on the moon had to be rejected, mainly because they caused impossible increases in the mean motion of the perigee. An equatorial ellipticity of the sun's mass,

[7] *Annales de la Faculté des Sciences de Toulouse*, 1907.

[8] *Diss., Freiburg* i. Br., 1912.

[9] *Amer. Jour. Sc.*, Vol. 29.

combined with a rotation period very nearly one month in length, appeared to be the best of these hypotheses. The obvious objections to it are, first, that such an ellipticity, small as it can be (about 1/20,000), is difficult to understand on physical grounds, and, second, that the rotation period of the nucleus which might be supposed to possess this elliptic shape in the sun's equator is a quantity which is so doubtful that it furnishes no help from observation, although the observed periods are well within the required limits. Dr. Hale's discovery of the magnetic field of the sun is of interest in this connection. Such a field, of non-uniform strength, and rotating with the sun, is mathematically exactly equivalent to an equatorial ellipticity of the sun's mass, so that the hypothesis might stand from the mathematical point of view, the expression of the symbols in words being alone different.

The last-published hypothesis is that of Professor Turner,[10] who assumes that the Leonids have finite mass and that a big swarm of them periodically disturbs the moon as the orbits of the earth and the swarm intersect. I had examined this myself last summer, but rejected it because, although it explained the straight line appearance of the curve of fluctuations, one of the most important of the changes of direction in this curve was not accounted for. We have the further difficulty that continual encounters with the earth will spread the swarm along its orbit, so that the swarm with this idea should be a late arrival and its periodic effect on the moon's motion of diminishing amplitude; with respect to the latter, the observed amplitude seems rather to have increased.

The main objection to all these ideas consists in the fact that they stand alone: there is as yet little or no collateral evidence from other sources. The difficulty, in fact, is not that of finding a hypothesis to fit the facts, but of selecting one out of many. The last hypothesis which I shall mention is one which is less definite than the others, but which does appear to have some other evidence in its favor.

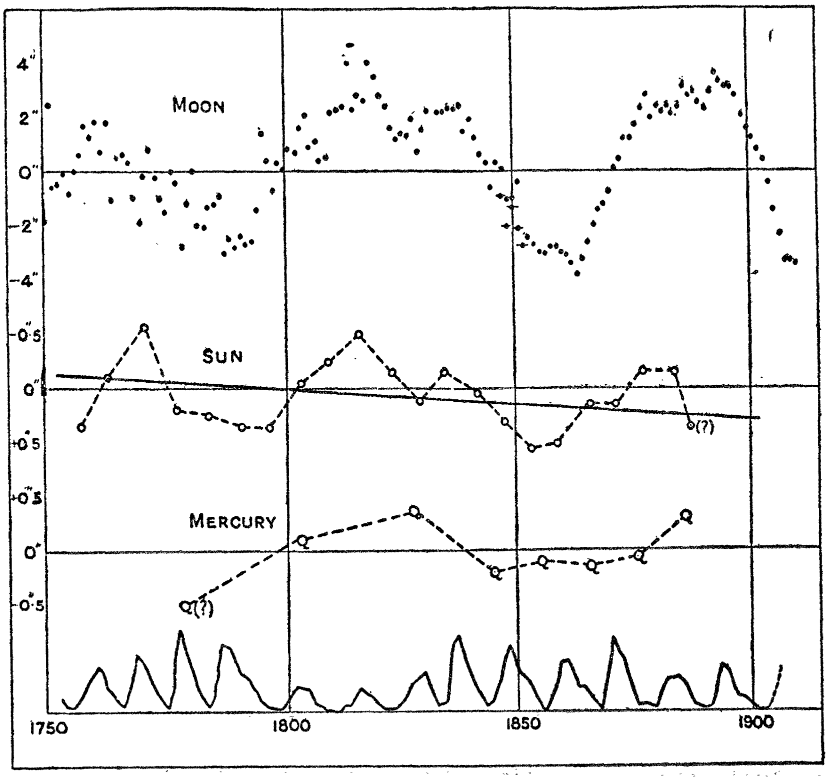
[10] *Monthly Notices*, December, 1913.

The magnetic forces, mentioned above, were changes in the *directions* of assumed magnetic fields. If we assume changes in the intensities of the fields themselves, we avoid the difficulties of altering portions of the moon's motion other than that of the mean motion. We know that the earth's magnetic field varies and that the sun has such a field, and there is no inherent improbability in attributing similar fields to the moon and the planets. If we assume that variations in the strength of these fields arise in the sun and are communicated to the other bodies of the solar system, we should expect fluctuations having the same period and of the same or opposite phase but differing in magnitude. It therefore becomes of interest to search for fluctuations in the motions of the planets similar to that found in the moon's orbit. The material in available form for this purpose is rather scanty; it needs to be a long series of observations reduced on a uniform plan. The best I know is in Newcomb's "Astronomical Constants." He gives there the material for the earth arranged in groups of a few years at a time. The results for Mercury, given for another purpose, can also be extracted from the same place. For Venus and Mars, Newcomb unfortunately only printed the normal equations from which he deduces the constants of the orbit.

On the screen is shown a slide which exhibits the results for the earth and Mercury compared with those for the moon. In the uppermost curve are reproduced the minor fluctuations of the moon shown

earlier; the second curve contains those of the earth's longitude; the third, those of Mercury's longitude. [By accident the mean motion correction has been left in the earth curve; the zero line is therefore inclined instead of being horizontal.] It will be noticed that the scales are different and that the earth curve is reversed. In satellites the same way but to different degrees.

The lowest curve is an old friend, that of Wolf's sunspot frequency, put there, not for that reason, but because the known connection for the last sixty years between sunspot frequency and prevalence of magnetic disturbance enables us with fair prob-



spite of the fact that the probable errors of the results in the second and third curves are not much less than their divergencies from a straight line, I think that the correlation exhibited is of some significance. If it is, we have here a force whose period, if period in the strict sense it has, is the same as that of the effect: the latter is not then a resonance from combination with another period. We must therefore look for some kind of a surge spreading through the solar system and affecting planets and

ability to extend the latter back to 1750. With some change of phase the periods of high and low maxima correspond nearly with the fluctuations above. The eleven-year oscillation is naturally eliminated from the group results for the earth and Mercury. One might expect it to be present in the lunar curve, but owing to its shorter period we should probably not obtain a coefficient of over half a second. Notwithstanding this fact, it is a valid objection to the hypothesis that there is no evi-

dence of it in the moon's motion. Reasons may exist for this: but until the mechanism of the action can be made more definite it is hardly worth while to belabor the point.

The hypothesis presents many difficulties. Even if one is disposed to admit provisionally a correlation between the four curves —and this is open to considerable doubt— it is difficult to understand how, under the electron theory of magnetic storms, the motions of moon and planets can be sensibly affected. I am perhaps catching at straws in attempting to relate two such different phenomena with one another, but when we are in the presence of anomalies which show points of resemblance and which lack the property of analysis into strict periodic sequences some latitude may be permissible.

In conclusion, what, it may well be asked, is the future of the lunar theory now that the gravitational effects appear to have been considered in such detail that further numerical work in the theory is not likely to advance our knowledge very materially? What good purpose is to be served by continuous observation of the moon and comparison with the theory? I believe that the answer lies mainly in the investigation of the fluctuations already mentioned. I have not referred to other periodic terms which have been found because the observational evidence for their real existence rests on foundations much less secure. These need to be examined more carefully, and this examination must, I think, depend mainly on future observations rather than on the records of the past. Only by the greatest care in making the observations and in eliminating systematic and other errors from them can these matters be fully elucidated. If this can be achieved and if the new theory and tables serve, as they should, to eliminate all the known effects of gravitation, we shall be in a position to investigate with some confidence the other forces which seem to be at work in the solar system and at which we can now only guess. Assistance should be afforded by observations of the sun and planets, but the moon is nearest to us and is, chiefly on that account, the best instrument for their detection. Doubtless other investigations will arise in the future. But the solution of the known problems is still to be sought, and the laying of the coping stone on the edifice reared through the last two centuries can not be a simple matter. Even our abler successors will hardly exclaim, with Hotspur,

> By heaven, methinks, it were an easy leap
> To pluck bright honor from the pale-faced moon.

They, like us and our predecessors, must go through long and careful investigations to find out the new truths before they have solved our difficulties, and in their turn they will discover new problems to solve for those who follow them:

> "For the fortune of us, that are the moon's men, doth ebb and flow like the sea, being governed, as the sea is, by the moon."

E. W. BROWN

BOTANY IN THE AGRICULTURAL COLLEGE

FIVE years ago, there was, I believe, no college in the United States which required that plant physiology be studied by any student of agriculture. There were a very few colleges in which it was possible for students of agriculture to take as much as one year's work in this subject, but the number of such places was exceedingly limited and remains so. The college of agriculture of the University of the Philippines was founded at that time; and having a free hand in planning its course of study, I provided that every student not only could but must take one full year of plant physiology, and that students taking the course regularly must have this year of physiology before being admitted to the study of agriculture itself.

There were several reasons for taking this rather radical step. Decidedly the strongest